Stadtman, Thressa 2001

Dr. Thressa Stadtman Oral History 2001

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Thressa C. Stadtman, Ph.D.

This is an interview with Thressa C. Stadtman conducted on January 23, 2001, in her office on the first floor of Building 3, National Institutes of Health, Bethesda, Maryland. The Interviewer is Dr. Buhm Soon Park.

Please note: this interview discusses journals and photographs and has been edited as closely as possible. Please contact history.nih.gov for any clarification.

Stadtman: [Beginning of tape discussing journal articles and photographs]….He worked on bacteria that decomposed sulfur compounds, and they found things in Yellowstone Park, in the hot springs. We're using this as a source because we have to write about this professor, an obituary. That's when I really started, before I went to California. That's this thing that I gave you, the re-grant. Well, I do have the year. Well, this is the isolation of this organism that I've used subsequently. This thing, we went and got samples of the black mud in San Francisco Bay. At that time it was really quite smelly. So you want me to circle these things?

Park: Yes.

Stadtman: So I would think this is one because this organism *Clostridium kluyveri* was named after Albert Kluyver, the picture I just showed you, with whom Horace Barker had studied, and so we named it after him. And then, subsequently, this has been the source of a lot of the selenium-dependent enzymes and vitamin B12-dependent enzymes, so it's been very useful. Well, all of these were. These were parts of my Ph.D. thesis. I suppose eventually you'll want the copy of the thesis.

Park: Oh, yes.

Stadtman: I don't think it's even bound. This is what I did when I first came to NIH. I really never quite liked it all that much, this business of this cholesterol metabolism.

Park: Were you in Dr. Anfinsen's lab?

Stadtman: I spent the year at Harvard. And when we got to Boston, Earl had a fellowship and I didn't, so I went shopping for a job. I found this job in Anfinsen's lab. And then he was the one who was invited to start this whole Heart Institute. So I spent a postdoctoral year. Earl was offered lots of positions at universities, but we both couldn't have positions at the same university. When we both were offered full staff positions in the Heart Institute, Earl said okay. That wasn't his first choice.

Park: He gave me letters of the time, the letters from a number of universities asking him to come.

Stadtman: We would go for interviews. I would be offered something in home economics or a laboratory. It was nepotism really, with two people in the same family. Even before that, during the war, when Earl and his brother were working together, they had to be separated because two brothers couldn't be together. So I guess that this started the whole thing. This is probably a good one to circle. It's the basis for a lot of things that became B-12. We did a lot of things with students, but this was, that's one of the early ones on B-12, vitamin B-12. Now, this again was B-12 involved in methane biosynthesis, so that was very new.

Park: Do you usually carry projects at the same time, or do you finish one and then go to the next one?

Stadtman: Well, you have students working on different things. It depends.

This was something given at the New York Academy of Science. Both of these were the first time we really had emphasized that. So here are two very different kinds of things that vitamin B-12 is involved in, all of those. That other small protein is the one that eventually became a selenium protein, you know, so all of these things are B-12. I was involved quite heavily in the vitamin B12-dependent reactions because that was new. This was one of the new ones that established what was going on. There's another that tells you where specifically the vitamin B12 is and how it's working. We have always been interested in how these things work. This was another one. This was a student I had from Germany. Lin Tsai, the organic chemist. He's now emeritus in the Laboratory of Biochemistry, at NHLBI, but he's in my group. He's been here many, many years, and he's collaborated when you needed identification of unknown compounds, and he always collaborated on these things.

I remember going to this meeting up in Montreal. It was kind of amusing. That's the other one. This guy was from England originally. He worked in my lab. Hsiang-Fu Kung came from Taiwan. And this, again, was another role, another enzyme, nicotinic acid. So Earl worked on and had students working on the nicotinic acid metabolism much earlier, and then we got into it because of the B12 and the B12-dependent reaction. Klung was very good. He's now--he went to Singapore for a while, and I think maybe he's in Hong Kong. That was a big review article that I was asked to write. I suppose you want those.

Park: Oh, yes.

Stadtman: It was one of the early vitamin B12 reviews. So here, pyridoxal. This is another vitamin, of course, and also involved in B12. So this was just the identification of an organism *Clostridium barkeri* we named after Barker. Earl may have pointed that out to you because his name and my name are both on this. Ira Pastan, who's up in...

Park: Yes, Cancer Institute.

Stadtman: He was a postdoc in Earl's lab at that time. So this was the beginning of the seleno-protein business.

Park: 1973.

Stadtman: David C. Turner was a postdoctoral fellow in my lab. C. van der Drift came from Holland, from Delft, originally Delft, and John J. Baker British and the Dutch guy were both involved in this detailed paper. It was kind of a review thing. Belinda Seto now has an important job in Building 1. At first she was in charge of minorities, and now she's liaison to Congress and all kinds of things. So this was really a crucial one. We were the first to show that selenium in…. enzyme is this particular amino acid, which is just a selenium analog of cystine, which is a sulfur compound. This guy (Joe Nathan Davis) was my technician, who then got his B.S. degree at George Washington University, and he showed that this organism grew on formate.

I suppose that one's nice. You could show that you really had to have selenium for growth of this organism. This was important for establishing the structure of the enzyme and where the selenium was. And I did collaborative stuff. I would take the enzymes to Zurich, and Duilio Arigoni[RLL1] was the chief of one of the big laboratories in Zurich, the technische hochschule[RLL2] and this is János Rétey in this lab. So they were doing absolute configuration of the products. This is all stereochemistry. This was another review, I suppose. I remember, I was invited by Alton Meister who used to be here at NIH for a long time. Then he was in Cornell Medical School until he died. So this is, again, the German, this Arigoni and Rétey.

Park: When you are the last author, do you usually write papers?

Stadtman: Actually, the way it worked is Rétey, I took the… Rétey did some of the work, and we wrote the papers together. Arigoni was head of the lab. His name went on every paper. His contribution was to take me for lunch. No, he read the papers, too. But that was the way he was brought up. He was Italian but in a German university. It's the way they do it.

This is nice. *[NB: The organism is Methanococcus vannielii and was isolated by Terry from mud that she took from San Francisco Bay while a graduate student. It was named after CB van Niel. Van Niel was a student of and assistant to Kluyver in Holland and extended and popularized Kluyver's concept of the unity of biochemistry – especially after he moved to the US and the Stanford Marine Station. Perhaps most importantly, he had a tremendous influence on many scientists around the world who took his renowned course on bacterial physiology and biochemistry. He taught the course from 1938 through 1962. Arthur Kornberg took the course and often stated that he was profoundly influenced by it.*] We had the selenium-dependent drugs made. Hidehiko Tanaka was one of my first postdoctoral fellows from Japan, who now is the dean of the College of Agriculture in Okayama, Japan and there are two successive people. They're all three now in the same Okayama University. He reminded me once that it was more than 25 years ago he was here, was always taking pictures of the rhododendrons in the garden.

They had these series of meetings. I belonged to sort of two of these clubs, selenium and biology in medicine, and then organic selenium and telurium compounds. These were organic chemists primarily. But then they finally decided, we'd better invite some of these selenium biochemists to these meetings, so each time they would have one--that was in 1979 in France--they wanted to hear about the selenium-dependent enzymes. That was the first one I was ever invited to. But then, since, I have been invited to all the other ones. They come about every three years.

Let's see now, this is Shigeko Yamazaki. She was a very marvelous Japanese postdoctoral fellow, and she proved, she isolated... I should have her paper. Her name was on it alone for showing that it had nickel and selenium in the enzyme. Maybe I have it. This was this thesis at George Washington University.

This is the other club, selenium and biology in medicine, those meetings were every three years or so. It was always an honor to be invited to those things. In Mosbach, Germany, they have yearly meetings. This was the 32nd, and I was invited to give something about enzyme catalysis with selenium, the selenium role in it. So I guess that's somewhat--it's an honor. Wei-Mei Ching originally from Taiwan, worked in the lab, actually about five years, and she was very nice in showing the selenium-containing TRNAs, and so that's important.

Park: You seem to have a lot of international fellows.

Stadtman: Yes. This person, Maris Harmanis -he's a big wheel now in Sweden, in Pharmacia. He's been director of research and a big part of Pharmacia. He came before he even had his Ph.D., and his girlfriend, Åsalie Hartmanis was at the Swedish Embassy, so he came with her. They were living together like lots of Swedes do. He wrote me a letter, could he come and work in my lab? But I felt finally embarrassed because I wasn't paying him anything, and I finally got a little money to support him later on. He really took to biochemistry and to enzymology. And then he came back and spent a year in the lab after he got his Ph.D. in Sweden, and he's been a friend ever since. So it turned out that this was not a real seleno protein, but it's important. It's an important thing.

And Yamazaki again. She did very nice work. I put all of these names on. This was another one of the selenium and telurium meetings. That one was in England. So this summarized a lot of the research. And then they identified two different seleno nucleosides. These are really in the TRNAs. That's important. Mark Sliwkowski came to me from the University of North Carolina. He was very, very good, and he did very nice work. And this is a nice one. It showed the hydrogenase.

Yamazaki showed that the hydrogenase--this is an enzyme that reacts with molecular hydrogen--was really a selenoenzyme and made stereochemistry.

Joe Davis was my technician from about 1964 or '65 until two years ago, when he retired. Later, I was invited to a meeting in southern Germany, some rather famous resort town. And, again, they published the papers. There's a bunch of things, like those two red books. Those would be something from one of the meetings. These come out in sort of semi-hard bound prints from meetings. Hartmanis did very nice stuff on this enzyme, and it is not a selenoenzyme. This started the collaboration with August Böck in Germany. He said, "We have the gene for one of these enzymes that has selenium in it, and we'll determine the structure of the gene, and you need to show that." So we proved that there was selenocysteine. It took quite a while to get the C points of the protein to prove that it matched the message code that tells you to put selenocysteine in. We've been collaborating off and on since '86. And this was the first to prove, how do you put selenocysteine in Boa protein, and it turned out the codon in the gene is TGA, which normally says stop. So there was a dual use of this codon which nobody had ever expected before. And the moment when Böck could show that the TGA in that enzyme, which is a dehydrogenase at the same time, people in Scotland had accidentally discovered a gene from some mouse sequence they were doing, and they had this, and here's a TGA that's in the internal structure. Leopold Flohé in Germany, had earlier discovered the sequence of the protein. He belonged to a big pharmaceutical company, and a lot of effort was put out. And lo and behold, where the selenocysteine and the protein was matched that codon, so eureka! So from two different, completely different sources of bacteria in a mouse chain, there is a way it goes in. But, it took a lot of work from August Böck's lab for the chemistry of getting rid, getting that particular seleno amino acid structurally produced so it could go, so it could be put on a special transfer amino acid TRNA. A lot of beautiful work has been done with him and his students, but we started the thing. Then Mark Slikowski, he did some nice things in the amino chemistry of this protein that we first worked with. David Grahame, who first worked with me and then got independent support from a Chicago gas company. Originally, I was invited to see if they would like to collaborate on research to try to make either hydrogen or methane from surplus plant materials, so they grew some sort of plant that grows luxuriantly in those swamps and canals in Florida, and then there would be certain groups of microorganisms that get, decompose these complex carbohydrates to simpler things in another set. And, finally, the methane bacteria can convert the simpler products to methane or they could keep it as hydrogen. And at that time, they were supporting this guy David Grahame to work in my lab, and some other people, and they calculated that whether you decided to keep it as hydrogen or as methane, it could account for about 5 percent of the energy need of this country. And as soon as oil got cheap from OPEC, all of this research support was canceled. And now California would like to...

Park: Oh, yes!

Stadtman: This is what they do, you know. Instead of continuing it at a modest amount, they'll stop it because it's not competitive. And then, when they need it again, "Oh, why didn't you scientists find out all this?" It's the stupidity of the people who make the rules and the laws. Never think about the future.

Park: That's right.

Stadtman: So there you are. Anyway, David Grahame did very nice things and, again, proved that one of the big intermediates in this whole process, methyl group, is put on this B12 compound, and that's one of the precursors. So he did beautiful work. And he's over across--he's a professor at the Department of Biochemistry at the Uniformed Services University of the Health Sciences. All of these were very nice. And it all came about initially by support, and then he took a job.

Eddie DeMoll was in the lab and did nice things. Mark Sliwkowski was particularly--he did the sequence around the selenocysteine. He was the one who told me he would have made a marvelous professor at a university, very well spoken, very bright and everything. He said, "Terry, I'm not going to take the chance of taking a job as an assistant professor and applying for a grant and not getting it." At that time it was very hard to get grants. Now, what do I do? So he went--he and his wife. I'm sure they're very rich now and they both work for Genentech, and she has a superior position and he has a big position in Genentech . And it's too bad, but, you know, he's done very well.

Park: He may want to come to NIH, working here.

Stadtman: They both worked here as postdoctoral fellows. Mary was in Cancer Institute and Mark was here. But they did very well and Genentech is one of the really good companies that survived. So, anyway, he did nice things on that.

We've had joint papers with this man Bach. This was a review type of thing. And Milton Axley as a geneticist, trained, who came to my lab. Böck made a mutant of this enzyme that had cysteine now instead of seleno cystine, and when Axley [sp.] studied that, the sulfur just didn't work instead of selenium, so it was a good example. I think the reprint is over here a little further. But it was a very crucial. Well, this one, the purification which he did in our anaerobic lab, Graham showed him how to purify. As a geneticist, he didn't know how to purify proteins, but David would have... Somebody had to stand on the outside while you worked in that anaerobic web. You've seen that lab?

Park: No, no.

Stadtman: I'll show you. So David coached him, this geneticist, how to purify the protein, then Böck engineered the mutant and Axley did the crucial, purified and showed that the sulfur just didn't work instead of selenium. So that was a very important paper. That enzyme was so sensitive to oxygen that nobody could purify it out in the regular rooms. That's a lecture I had to give on the occasion of this, oh, one of these awards that was...then Gregory Garcia spent time in my lab and isolated two of these, the genes for the seleno protein. We have almost no genetic work done on clostridia, so that was a hard job. And then we did collaborative stuff with this man Dolph Hatfield who's up in Cancer, works specifically on TRNAs. So, finally, we did our share and showed that the seleno cystine in the formate dehydrogenase matched the TGA in the gene. And, again, Joe Davis did all these amino acid and analyzed those things and was very good.

So, Axley and Grahame did a lot of nice things on the enzyme. This was one of the, I guess, a lecture made in Korea.

Park: Korean journal, yeah.

Stadtman: Yeah. I was invited to this meeting discussing vitamins and biofactors in life science. This nice Japanese emeritus , Nobuhiko Katunuma, decided we had to have some sort of an international association to get all of this wonderful information available at the basic level so that clinicians would know about it. So, I did what I could to help, and we set it up. And they organized this first international meeting on vitamins and selenium and things like that.

Park: Heavy metals.

Stadtman: How are they involved. And they had about 800 people at that meeting in Kobe fore the big earthquake, and about 600 of them were clinicians. That was an important meeting and that was the first. I ran the second one in the series in San Diego, actually in '95, no, in '96. And then another one was run in Germany, and then another one just this past year in Italy, so we're in the fourth cycle of those.

Park: By that time, were there many biochemists working on selenium?

Stadtman: Yes. This was... Lots of people were beginning. There was beginning to be a lot more interest because it was now 20 years. At first it was really very few. And in the mammalian system, there was only one enzyme known for a long time. And then, actually, we were the one that discovered that mammalian thioredoxin reductase is a selenocysteine containing protein accidentally. Where do I find this? That's the one, an enzyme that people knew about a long time. Ah-ha, it is this guy, Takashi Tamura. He came and he wanted to--more of a chemist, an organic chemist. He'd been trained in Japan. He was a young professor already. He had the position. So we knew that there was supposed to be one special enzyme in this adenocarcinoma, some very unusual, but the gene sequence that somebody over in Cancer had determined said, "There ought to be selenocysteine," and that would be very interesting. So, anyway, Takashi made, got the cell line and learned how to grow it and added radioactive selenium and isopurified a protein about the right size, and it didn't have any of the co-factors of the thing we were looking for. And eureka! Here it was, a mammalian thioredoxin reductase, which nobody had suspected. A guy that I know in Sweden called me one day. What a surprise! So that really was a highlight of this. And it's serendipity and science. Something you weren't looking for proves to be so important, you never would have dreamed of it. Nobody dreamed about it. Other people had purified this thing from rat livers and from things. No clue.

So then this Russian, Vadim Gladyshev, in my lab and this Chinese guy, each had preparations from different sources and we could get them sequenced and show where the selenium, that selenium is. But he purified and proved that it was the selenocysteine in it, and he made two different derivatives. He was marvelous. He was here only, allowed to be away only one year and one month, but every experiment was finished, and no kinds of loose ends. Found a radioactive protein, derivatized it and hydrolyzed it and made two different derivatives and showed what they were, identified chemically, and then looked at the co-factor. Well, the co-factor gave a little clue, and then did another step. And each time the experiments were conclusive. And we could write the finished paper with this brand-new information.

Park: That's amazing.

Stadtman: So, there are people like Matt Wolf who are--it's wonderful to have them in the lab. Anyway, so Takashi Tamura is the third one from this University of Okayma and we've just collaborated in writing a review for *Methods in Enzymology*. He sent me part of it. We've been fixing it back and forth.

Park: Approximately how many fellows or workers do you have at one time?

Stadtman: Oh, four or five.

Park: Four or five. Including the technicians?

Stadtman: Well, at the moment I have no techs, but for this long, long time, this man, Joe Davis. But he was the helper for the general group of us, so he was not strict purely and simply... He ordered supplies for us and kept the instruments going, and he was very good at keeping things running, also running an amino acid analyzer for me and for other people.

Park: Mm-hmm, indispensable.

Stadtman: And then he did a little research problem on his own that was of interest, purified a protein. We don't know what it does, but, anyway, Joe Davis was very good.

David Grahame still keeps me up to date on what he's doing, and it's a continuation of those original things. So this was the other big thing that... So how do you... One of the important things about getting able to put seleno cystine into proteins, how do you make it? So one of the chemical precursors, we knew that it went into TRNAs and also went into proteins, and so... And collaboration with Robert Balaban, who's now the Scientific Director of NHLBI, our institute, was always doing lots of NMR studies. One of his people identified… the labile selenium donor, and we've identified the selenophosphate. So that's very important. And the proof of it was in collaboration with an organic chemist, Richard Glass from Arizona who spent his sabbatical year in my lab, and he was interested in selenium. He was a sulfur chemist and interested in selenium. So, where is this paper showing the proof of the structure? Selenophosphate. I must have skipped it someplace here. There we are. This is the proof of the structure, selenium bonded to phosphate, so it's selenophosphate. He made the chemical compound chemically, and then we could show that in all kinds of procedures and also in a biological system, it was identical with the radioactive stuff that the enzyme made, so that was very important.

We first had a very good idea of what had to be by these experiments, this is very diagnostic. But this stuff is so oxygen sensitive you can't make it and just add it to things. So we still--it's a big, important problem, though, since you and I make it, the bacteria make it. And do we make it each place it's going to be used or do we, are we able to condense it with something so we can circulate in the blood and take it to the next place, take it to your brain? Nobody knows. So that's one of the big problems, how some very oxygen-labile compound that's important and has to work in many different locations. There's a lot of the enzymes in, for example, in the rat liver, so there may be enough so that it's here and here and here and here, enough of it to make the precursor everyplace so you don't have to circulate it. There's a lot of interest in how it's controlled, and there's a lot of interest in things like that that have to be...

So then there are bacterial enzymes. We've known some of them earlier that have selenium in that's essential, and it isn't selenocystine. This is what one of my young postdocs now is working on and trying to identify. We think we know what it is, but we don't really know. So this was important. This was done with electron spin resonance studies. These were two Russians Sergei V.Khangulov and Vadim Gladyshev in my group, and Sergei V. Khangulov was the EPR expert, so these two Russian guys were working on that project . So then they showed that in formate dehydrogenase the selenium is coordinated to a molybdenum.

Those are more detailed studies. In those, I'd say we're plunged into them. This was—Ick Young Kim was in the lab for quite a while, from Korea. He did very nice work on this enzyme, and he could show that it's easy to find it in all kinds of rat tissues, so there's a lot of it in the animal. This is why I told you that one of the possibilities is that he could make this oxygen-labile stuff right where it's going to be used. You don't have to transfer it in the blood. So the location of the selenocystine was very important in that protein. And these two, the Chinese guy and the Russian guy, were both... This was a different paper. I circled the wrong one, but it's this one. This one is showing it. And he did also very nice studies, which we're trying to figure out how that enzyme really works. And then I had this nice woman, Heidi Walker, and this was an important study on how this enzyme that makes the selenophosphate works, and they have a system where you can do some exchange reactions with one of the intermediates and prove by isotope labeling that you get migration. It' s a very interesting procedure.

And then Kim did some nice stuff on ultimate effects of inhibiting selenite. Too much selenite is very inhibitory also. It's toxic stuff. And we have some clues as to toxicity. This would be a clue as to toxicity, and there's few known things but we really don't know a lot about why selenium is so toxic. In terms of the mechanism of how the selenium in one of these enzymes worked, we have three different interpretations, one from the x-ray analysis, one from something called XAS, x-ray absorption spectroscopy, and another from just enzymology. So, Heidi Walker did very nice experiments also on that enzyme, which--well, we still don't know exactly how it works.

Here's another one of these selenium and tellurium things. There was a meeting in Aachen in Gernmany, and I presented some of that work on this mechanism of selenophosphate.

Christian B. Allan was in the lab. He's more of an organic chemist, and he spent time on studying how selenium gets into those TRNAs. Gerard Mecus Lacourciere is here now. He's extremely good, and he's working on these proteins that deliver selenium directly to the enzyme that's going to make the selenophosphate, so you don't have to have these high toxic levels of free selenite. And this clearly, very... We call these delivery proteins.

The chemistry of this is very puzzling. I contact some of my sulfur and selenium organic depends on what kind of a compound is this. It's not--it acts like selenite but it isn't. What is it? Sulfur and selenium chemistry, just inorganic chemistry--there are a lot of things that nobody knows. So there are going to be a lot of surprises. This other young man, William Self, who came to me from the University of Florida about a year and a half ago, and he's discovered a new enzyme that has selenium in it.

This woman from Israel, Shoshana Bar-Noy made the mutant of this mammalian thioredoxin reductase, and when you put sulfur, again, instead of the selenium, it's almost inactive. And she's been studying some of the effects of this and what the enzymes really are. In any event, so, now Gerard Lacourciere has spent a month in Japan, in Kyoto, working with this group that I've known. Osaka [NB: Stadtman said "Kyoto" in the preceeding sentence but now correctly says the group is in *Osaka*] long time. And two of these guys are coming here next month to do some collaborative studies, again on these delivery proteins, and so that makes a very nice arrangement. And as I said, Gerard is an extremely good scientist. He's good at molecular biology and at the biochemistry and purifies. Both of these fellows are--they know all the methods very well. And so he expressed this and fixed up the gene so it can be translated in *e. coli*.

And then the Russian guy (Khangulov) succeeded in crystallizing this enzyme, the formate dehydrogenase. I guess I skipped over that. And the crystal structure was published, and the crystal structure suggested one way it might work, this one. So this is important. Crystal structure of the formate dehydrogenase published in *Science*. And Jeffrey C. Boyington who did the x-ray absorption thinks another way it might work, and then Khangulov, the

fellow who had done all these EPR studies had another idea, so you have three ideas of how it may work. And this is often the case, you know. Now you need some still other, something new on the horizon, some methodology we don't have at the moment, to figure out what's going on right at the site where this chemistry is taking place.

Park: I see.

Stadtman: Anyway, that's kind of skipping over rather quickly, but...

Park: Great, great.

Stadtman: We had a big interruption in my work on this, one of these delivery proteins from this methane organism that I isolated 40 years ago from San Francisco Bay mud, and I think this highly specialized one that doesn't like, doesn't compete with the sulfurs... I have to do some more work on that.

Park: Do you have a kind of diagram of the mechanism of selenium insertion or illustration?

Stadtman: Well, there would be several diagrams. Like the German group had worked out very nicely how the selenium is prepared to get into seleno enzymes as selenocystine. And then Gerard has, working on some ideas about pictures of how these delivery proteins may work. You know, we' ve got a lot of kinds of pictures. For example, in the sulfur, there's a lot of mimicking. **[Tape inaudible for about three minutes.]**

Park: I'm thinking of what images or objects that I can put on the exhibits that represent your studies and captures the essence of your studies. So that can be from the published articles, but also from the research notes or other drawings or...

Stadtman: Well, we make a lot of these things for slides, you see, and so Gerard has made some up with the stuff he's doing, these delivery proteins, for slides, and then I have some reproductions from the germinal work primarily done...

Park: So, just to have in mind that .

Stadtman: I have a nice colored picture someplace of how these, this first enzyme that I worked on, how it the selenocystine worked, which is kind of nice for the history.

Park: Oh, yes.

Stadtman: See, I have a lot of notebooks there with the slides. If I could lean over you, I'll show you what one of these colored things looks like. I've been hunting and hunting for the originals, I can't find anything. I must be looking right at it. Here it is.

Park: Yes.

Stadtman: That kind of thing.

Park: Right.

Stadtman: So I have to find the originals from which I copied those--I don't know. They're floating around someplace--so that this, the central part of it was the protein that had the selenium in it, and it showed what it was doing. But that's what you want.

Park: Right.

Stadtman: Because these kind of things you just need pictures like that.

Park: That's right.

Stadtman: And I can dredge up... I have all of the reprints of my German collaborator and lots of things that they did. My name is not on the papers for the papers for some things. But in some of those papers there'll be a nice scheme.

Park: Right. Do you have any collection of your reprints?

Stadtman: Yes.

Park: Like a book that I can...

Stadtman: Well, they're in all these. The real old ones, are out in the corridor. For a while I had a secretary who put them in notebooks. Somebody did these, but they're not--nobody does them for me anymore. So those are the very first ones.

Park: That's nice.

Stadtman: So, anyway, I guess these...

Park: Can I borrow these and make a copy?

Stadtman: What I was going to say is maybe I'll just give it to you. I don't know. I think I have essentially all of these things in the files someplace. Then, let's see. This says... Now, these are from '76, '76 to '82, '76 to '82. I guess these must be earlier. These are probably in that one. Maybe. Let's see. I guess '67. These are when I first came to NIH. There's no point in you copying them because I know there are duplicates of all of these in a file. You could look to see which ones you want. If you did that, then I could pull them out of a file.

Park: Okay.

Stadtman: So there's two. Some of them are in that red book. Here's one, see what's in that first book. You can compare. These are easier to pull off.

Park: Yes.

Stadtman: You see, at one time all these were lectures.

Park: Your lecture?

Stadtman: Yeah. It seems to be mixed in here. Let's see when I was doing. These are notes, obviously, for lectures. I wonder if this is a dissertation. That's in references, references I've made from the literature. These are references. This is--these are things from others. I gave a series of lectures so I know what they were for. When we were in Germany once, I gave a series of lectures to a course. This was the lecture I gave at that Montreal meeting, so these papers.

Park: It was published.

Stadtman: Yeah, it was published.

Stadtman: Yeah. So that's my notes. I sometimes used to write up fairly well. Slide 21. Let's see. This was what the lecture was. Slide 17 made notes about what I was going to say. You know, acid reductase. I don't think you'd want to pore through all of that. Okay? These are the things that you don't keep, I guess.

Again, some research seminar, and the slides show. Now, that's a little history thing. It's *clostridium* that was isolated from San Francisco black mud. History. We discovered all these things. There's where the enzyme worked and that group _____ so soon. Nicotinic acid fermentation. That must have been a lecture on nicotinic acid fermentation, slides. You see it's kind of fragmentary. Well, you know, you make yourself notes. This is the sort of thing that I suppose during the move will have to be thrown.

Park: Well, throw it to me. Let me tell you, eventually your documents and papers and other things...

Stadtman: Correspondence.

Park: Yeah, correspondence.

Stadtman: There's a section down there on correspondence.

Park: I'd love to...

Stadtman: That's what you want, huh?

Park: Yeah. It would be eventually sent to the National Library of Medicine. There is a History of Medicine Archival Center.

Stadtman: Yeah. A lot of the trivial things, though, get into correspondence. Somebody writes to you and wants . That's not real history.

Park: You can sort it out. But our hope is...

Stadtman: What I have at home would be nice. When I was elected to the National Academy of Science, there weren't very many women. And I was really impressed. In fact, I have at home a big, thick thing like that of letters of congratulations from lots of scientists--all the scientists I knew plus a lot of people I didn't really know, so that's the sort of thing, you know.

Park: Yes, yes. And do you have any correspondence that you exchanged ideas, you know, kind

Stadtman: There are some of those.

Park: That shows the kind of emerging stage of, the formative stage of ideas that...

Stadtman: Yeah. Well, for example, I showed a recent one to Lacourciere, the people in Kyoto, about two years before we started because it was obvious that that must be what was going on. Yeah, I can sort through those and throw out just the silly little things about somebody wanted a compound, you know. That's not very interesting.

Park: Yeah. That's not very interesting. Yeah. You know.

Stadtman: Now you'll get them on the e-mail.

Park: Yes, that's right.

Stadtman: And those get thrown away, the ones that I don't want. I just had an e-mail from a woman in Poland. She would like to come and work in the lab. Then, in the next e-mail, she wants money. She doesn't have any money. So I said, "Well, that's a little harder to do. If you'll send me your C.V. and everything, then I can try to find out." But these people think that, you know, they can just write and ask, and she's somebody who's been working in the field 10 years or something like that, and they don't really try this on their own.

END OF INTERVIEW

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